

BOOK REVIEWS

SCIENCE AND SUPERNATURE: A CRITICAL APPRAISAL OF PARAPSYCHOLOGY by James E. Alcock. Buffalo, NY: Prometheus, 1990. \$24.95, cloth. L.C. 89-70033. ISBN 0-87975-548-2.

In 1987, the journal *Behavioral and Brain Sciences* (BBS) published a debate on the current status of psi research. Ramakrishna Rao and I were selected to make the case for psi research, and critic James Alcock was chosen to attack it. Comments were published on these two "target articles" from forty-nine respondents of varying professional backgrounds who have some interest or expertise in parapsychology. The authors of the target articles then wrote replies to the commentaries. Alcock's target article and reply comprise the first half of *Science and Supernature*.

The second half of the book consists of a background paper Alcock wrote for a committee set up by the (American) National Research Council (NRC) to evaluate certain controversial techniques for enhancing human performance. One of the topics chosen was parapsychology, even though few parapsychologists believe that psi currently has much value as a performance enhancer in the real world. The chairman of the parapsychology subcommittee was Ray Hyman, and it is not surprising, therefore, that the committee's evaluation of psi research was largely negative. Alcock's background paper was devoted exclusively to critiques of remote-viewing and random-number-generator (RNG) research. Appended to the paper were short critiques of each of Helmut Schmidt's major published experiments through 1986.

I was personally involved in the context of this part of the book as well. In 1985, when I was at the University of Utrecht in The Netherlands, I was approached by a staff member of the European Research Office of the (American) Army Research Institute to write an evaluation of the research literature in parapsychology. I was given no details about how the report would be used, except that I was led to believe it might have some influence on military funding of psi research.

Although my evaluation contained numerous criticisms of methodology, I concluded that these criticisms were generally not serious

enough to invalidate the research as evidence for a genuine scientific anomaly of potentially great importance, worthy of government support. (I felt that to write too favorable a report, in addition to being intellectually dishonest, would leave me, as a parapsychologist, open to charges of bias and would reinforce the stereotype of parapsychologists as incapable of judging psi research critically and objectively.) However, my report obviously was not critical enough for certain powers in the government. Alcock reveals in the Introduction of his book that the purpose of the NRC committee was to provide a "second opinion" (p. 4) on the research I evaluated. For this reason Alcock's background paper frequently refers to my report.

In general, I would characterize these references as accurate but selective. Although frequently citing my criticisms of psi experiments, Alcock mostly ignores my criticisms of the critiques of these experiments and other remarks favorable to the pro-psi viewpoint. In the remote-viewing section, for example, he leaves out my point that the SRI remote-viewing experiments remained significant when reanalyzed by more appropriate statistical techniques. He also ignores my methodological criticisms of the unsuccessful remote-viewing experiments of Karnes, a critic. In the RNG section, he makes a big issue of an analysis I reported suggesting that the results of the Princeton RNG research were attributable primarily to one subject (which does not denigrate their value as evidence for psi, anyway) while he ignores other analyses I reported, particularly one involving results in the "voluntary mode" of testing, which suggest that other subjects may have contributed as well.

In the Introduction of his book, Alcock uses my report to claim that the NRC committee already had access to a parapsychologist's views and thus there was no need to have one on the committee. This specious argument overlooks the fact that committee members and authors of background papers have different roles in the evaluation process. Balance is also needed in judiciously weighing the merits of the opinions expressed in the various background papers and of other data presented to the committee, and parapsychologists had no opportunity to participate in this part of the process.

Alcock concludes his Introduction with a brief discussion about a workshop on parapsychology sponsored by the U.S. Office of Technology Assessment (OTA) in which he and Hyman participated along with several psi proponents, including myself.¹ He at-

¹ See Office of Technology Assessment (1989), "Report of a Workshop on Exper-

tributes the poor showing of the critics in this exchange to their being outnumbered and not being aware that it was going to be a debate. I doubt that most objective readers will find these excuses persuasive. At least the critics had some representation on the OTA panel, which is more than parapsychologists had on the NRC committee.

Alcock's *BBS* paper contains the standard litany of criticisms one finds in the skeptical literature, except for a welcome deemphasis on speculative insinuations of experimenter fraud. (In his NRC background paper, however, he attributes such restraint to the fact that experimenter fraud is unlikely to be detectable in experimental reports, not that it is unlikely to occur.) He concludes that "... judgment should be suspended until there is at least some consistency among research findings from a body of irreproachable experiments, at least some of which are repeatable in [the] strong sense" (p. 28).

Although Alcock's arguments have not changed much from those in his first book,² there is a considerable toning-down of the hyperbolic rhetoric, and the presentation of his arguments has become more sophisticated (although still largely unpersuasive, in my opinion). Nonetheless, he found himself on the defensive with some of his *BBS* commentators (including some fellow critics) for speculating that the persistence of psi research is attributable to parapsychologists' being driven by a need to confirm metaphysical dualism. Several found this to be an inappropriate ad hominem attack. Although Alcock points out in his reply that he did not say this alleged obsession with dualism invalidates parapsychologists' research, the essence of the commentators' concern remained unchallenged. He suggests that they might have been led to overestimate the importance of this theme by the title he gave to *BBS* for his article, namely, "Parapsychology: Science of the Anomalous or Search for the Soul?". The title of the present book, *Science or Supernature*, is not likely to be any less offensive to parapsychologists, because it includes a cognate of the word supernatural in obvious reference to parapsychology as distinct from "real" science.

There are a couple of specific substantive points in Alcock's *BBS* paper that I would like to touch upon briefly. In challenging the

imental Parapsychology," *Journal of the American Society for Psychical Research*, 83, 317-339.

² See J. E. Alcock (1981), *Parapsychology: Science or magic?* Oxford: Pergamon Press.

argument made by me and other parapsychologists that critics bear some responsibility to show that the flaws they allege can actually account for experimental outcomes, Alcock says, "I can think of no area other than parapsychology where anyone has attempted to place the onus on the critic to demonstrate that an acknowledged flaw was both a necessary and sufficient cause of the effect" (p. 66). First, I don't believe anyone is arguing that the artifactual cause must be "necessary and sufficient," just plausible, and must have some empirical basis. Second, other areas (of psychology, at least) don't have armchair "critics" at all, but researchers collecting evidence for opposing viewpoints. When members of one camp point out alternative interpretations (what our critics like to call "flaws") of experiments by members of the other camp, they customarily do experiments of their own to empirically confirm the interpretations they prefer. The controversy over the proper theoretical interpretation of hypnosis is a good case in point.

Of course, as Alcock never tires of pointing out, parapsychologists likewise should do additional experiments to distinguish among the competing interpretations. This is frequently done, as in the more recent ganzfeld experiments. However, in a field where conclusions must depend on the evaluation of whole bodies of research rather than on single experiments, and where history shows that some sort of alternative interpretation can eventually be concocted for just about any experiment, assessments of the probable validity of these alternative interpretations are simply a necessity. Such evaluations are also sometimes helpful in deciding if an inconclusive body of research is worth following up.

Alcock places considerable stress in his *BBS* paper on the argument that parapsychologists use the presence of various "effects" (e.g., decline effects, linger effects, experimenter effects) to render the so-called psi hypothesis unfalsifiable. Although most parapsychologists understand that such effects, and post hoc analyses generally, are evidential only insofar as they are replicable or contribute to some consistent or theoretically meaningful pattern, I can see how this awareness might not come across to those from outside the field who read our journals. The Hyman-Honorton ganzfeld debate illustrates how such misunderstandings can arise when papers written for internal consumption get reviewed by outside evaluators. The message, it seems to me, is not for us to abandon post hoc analyses, but rather to be more careful in our discussion sections to make it clear and explicit what conclusions can or cannot be drawn

from them. I have been encouraged in my brief stint as acting Editor of the *Journal of Parapsychology* to see how many referees are picking up on this point.

Finally, I have been curious to see how critics would respond to my suggestion that psi research has provided evidence for an anomaly rather than for paranormality. My general impression is that they have rejected the anomaly claim, too, which serves to make their position more extreme and less defensible than it was before. Alcock, who devotes considerable attention to the anomaly concept in his *BBS* paper, clearly shares this opinion. However, in the course of defending it, he sometimes writes as if *anomalous* is just a less offensive synonym for *paranormal* and then uses the standard arguments against the old claim to undermine the new one. For instance, he writes that "even if paranormal were to be defined only in terms of anomaly, this would still lead to a dualism of some sort because of its independence from considerations of time and space" (p. 18). Perhaps he is still trapped by what I call the "traditional conceptualization" of the psi controversy.³

The critiques of the remote-viewing and RNG research in the NRC background paper draw heavily on the writings of other critics. The most thorough and original treatment is given to Schmidt's research. Only Schmidt's first set of experiments with prerecorded targets⁴ and the recent experiment with independent observers⁵ get grudgingly good reviews, which, considering the source, is tantamount to high praise. Many of Alcock's criticisms are more aesthetic than substantive. For example, he complains that the designs are sometimes needlessly complex and the same type of RNG is not used consistently.

The most serious criticisms by Alcock concern the randomness of the RNG output. Alcock echoes Hyman's concern that Schmidt's randomness tests may not be powerful enough to detect short periods of nonrandomness. He then adds a new twist that explains how this tendency could bias the results. He notes that Schmidt often waits until a subject begins scoring well before starting a formal session. If there is a short-term bias, Schmidt could innocently begin a formal test part way through the bias period and end it soon

³ See J. Palmer (1988), "Conceptualizing the Psi Controversy," *Parapsychology Review*, **19** (1), 1-5.

⁴ See H. Schmidt (1976), "PK Effect on Pre-Recorded Targets," *Journal of the American Society for Psychical Research*, **70**, 267-291.

⁵ See H. Schmidt, R. L. Morris, and L. Rudolph (1986), "Channeling Evidence For a PK Effect to Independent Observers," *Journal of Parapsychology*, **50**, 1-15.

after the bias vanishes and the scoring rate declines. These biased segments could then accumulate and lead to a significant total score.

This potential "optional-starting" artifact does not apply to many of Schmidt's experiments, particularly the more recent ones. For example, it is hard to see how it could affect studies in which the target sequences are derived algorithmically from random seed numbers because these numbers bear no simple relation to the relevant qualities of the output string.

As Alcock points out in his book, one helpful approach in evaluating Schmidt's ESP studies would be to examine the actual target sequences. Happily, Schmidt saved the raw data from his early precognition experiment,⁶ which Alcock and Rao both featured in their *BBS* papers. Schmidt kindly supplied me with copies of these data on disk, and I am currently undertaking several analyses of the randomness of the experimental target sequences. These analyses include (a) application of Good's Generalized Serial Test for the total trials completed by each subject in each of the two series up to the sextuplet level, and (b) summed chi-squares at both the singlet and doublet levels for successive blocks of sizes ranging from 24 to 200. I plan to report these analyses in due course, after consultation with Schmidt. So far, they provide little support for the optional-starting hypothesis.

Science and Supernature, despite its flaws, provides a useful complement to Hyman's recent anthology⁷ in giving a reasonably detailed account of external criticisms of major parapsychological research paradigms. (Hyman's book deals extensively with ganzfeld research, which Alcock discusses only briefly in his book.) Of course, readers who seek to draw some closure on these topics will want to get the other side of the story as well. In particular, I agree with Alcock that they should read the entire *BBS* exchange.⁸ *BBS* should be available at most university libraries.

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⁶ See H. Schmidt (1969), "Precognition of a Quantum Process," *Journal of Parapsychology*, 33, 99-108.

⁷ See R. Hyman (1989), *The elusive quarry*, Buffalo: Prometheus.

⁸ See *Behavioral and Brain Sciences*, 1987, 10, 539-643.